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We Ask, Does Psi Exist?

*But is this the right question and
do we really want an answer anyway?*

Abstract: *Although the question 'Does psi exist?' has become a recurrent and intransigent problem for psychological science, seen from a historical and social context, there appear to be reasons as to why no determined effort has been made to resolve the question. The sporadic exchanges from parapsychologists and critics appear only to reinforce the status quo: At most, it is agreed that some form of 'anomaly' has been established but there is no consensus about its nature. Yet such a defeatist stance shies away from the long tradition of research into spontaneous phenomena, as well as experimental research with dreams and hypnosis, all of which suggests that, if psi is real, it is not just an anomaly but has true information content and dynamic effects. This work is briefly reviewed and then, since much of the current controversy has centred on the ganzfeld technique, this is used as an example of the issues involved. The ganzfeld induces sleep-onset imagery during which randomly selected film clips are viewed by a sender as a source of psi-mediated information to be incorporated into the imagery. Our current development of the ganzfeld technique, known as the digital autoganzfeld, takes the work beyond 'anomaly status' since it enables real-time recordings to be made of apparently high-quality psi as it enters consciousness. Initial findings from this illustrate the complexity of the phenomena and the need for more adventurous designs in order to stimulate theory development. There is no doubt that technology now exists which would enable us to resolve the issues raised. Whether the area turns out to be a mixture of rampant fraud, artefact and subjective validation or to present genuine phenomena, a resolution of the issue may be seen as having unwanted implications for psychology — which may be one of the factors explaining why the controversy is left to continue as it is, unresolved. New questions are needed which take into account the natural context in which psi appears to occur, along with the support needed to reach clear answers.*

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One person who would answer the questions that form this paper's title in the affirmative is, of course, the founder of modern experimental parapsychology, J.B. Rhine. A few weeks before he died in 1980, Rhine said to his interviewer: 'We overlooked many things. We failed to know how to convince people. We assumed it would all be accepted.' Then he asked: 'What does the world need to know to remove this blockage to acceptance?' (Berger, 1988). In fact, John Beloff had given him the answer in part, some years earlier when he wrote:

Rhine succeeded in giving parapsychology everything it needed to become an accredited experimental science except the one essential: the *know-how* to produce positive results when and where required. (Beloff, 1973 p. 291).

Actually, Rhine fully accepted Beloff's critique but argued that progress would only come slowly by piecing together the variables that would eventually define the necessary and sufficient conditions for psi to occur. His disappointment lay in that he believed that sufficient evidence had been accrued to at least warrant the support of psychology in pursuit of this ideal (Rhine, 1972). Beloff's answer was thus for Rhine only a partial answer, but then I believe Rhine may have underestimated the complexity of the subject matter that he — and now we — are dealing with. To seek better questions and fuller answers is the quest of this article.

The Lessons from History

At least one part of the answer to Rhine's question actually comes in the form of the remit for writing this text. The editors felt that the JCS had been 'too favourable towards parapsychology' and 'this imbalance should be remedied'. Naturally, it might be asked why the status quo as regards parapsychology must *always* be maintained. The directive does also add a rider: that there should be 'scholarly articles on both sides of the parapsychology/skeptic debate which properly reflect all facts and findings'. Even so, the implication remains that the accused will certainly get a fair trial before being hung or at least left hanging.

This is understandable from the pragmatic viewpoint since the study of consciousness is itself a controversial area which may be in danger of losing its current forward thrust if it be diverted into dealing with a difficult and, in some circles, suspect area. Seen in the historical context this reaction is also nothing new. Indeed, a historical overview of this area does lead to the impression of it being a forum where the scenes and players change but the roles are essentially the same, and the last scene always stays the same by letting the issue remain unresolved. However, it would be unwarranted to conclude as some critics have done (for example, Blackmore, 2001) that given no present agreement can be reached on the phenomena's existential status and that the phenomena demand a standpoint, then this standpoint should be that they probably do not exist. That contrasting and polarized views should exist among experts who have spent their lives studying the very same human experience may be dispiriting, but it is hardly unusual: there is, for instance, still little agreement among experts

concerning the basic nature of hypnosis and dreams.¹ In the case of parapsychology, history suggests there may even be some powerful, and for some individuals, quite plausible reasons for maintaining this status quo, which is an issue we will later have cause to return to.

Virtually each century seems to have produced its doyens who would argue strongly that the phenomena were real while others would argue with equal conviction that they were illusory. Only certain Greek philosophers (notably the neoplatonists) seem to have been an exception by apparently having no problem whatsoever in seeing paranormal phenomena as part of a greater unseen natural order of the world (Dodds, 1971). By the 1600s paranormal abilities had come to be viewed in the context of witchcraft and apparitions but Joseph Glanvill, chaplain to Charles II, suggested that rather than demonic agencies being on the loose, some genuine paranormal ability lay behind all the superstition and thus he compiled the casebook: *Saducismus Triumphatus: Or, Full and Plain Evidence Concerning Witches and Apparitions* (1681). This was soon countered by Balthasar Bekker's *The World Bewitched* (originally published in Dutch in 1691 and in English in 1695) which revealed the magical tricks and natural causes leading to supernatural beliefs. Two classical works extending these themes were influential in the 1700s: Richard Baxter's book, *The Certainty of the Worlds of Spirits* (1691), and Charles Owen's *The Scene of Delusions*. In the 1800s, the physician John Ferriar wrote an impressive work, *An Essay Towards a Theory of Apparitions* (1813) which easily matches today's scepticism in seeking normal explanations of apparitions in terms of tricks, false memories and delusions. In 1848 came Catherine Crowe's much cited work *The Night Side of Nature*, documenting cases of apparent telepathic and apparitional experiences although not with the same critical ability that would come to characterize the work of the English Society for Psychical Research (SPR). Founded in 1882, the SPR is generally recognized as the first systematic attempt to bring some degree of objectivity to the subject by seeking witness testimony and evaluating the possible normal explanations for the experiences recorded. William James commented that were he asked 'to point to a scientific journal where hard-headedness and never-ending suspicion of sources of error might be seen in their full bloom, I think I should have to fall back on the *Proceedings of the Society for Psychical Research* (James, 1896/1972, p. 38).²

For some years psychology and psychical research shared a common platform. Henry Sidgwick, one of the founders of the SPR, was the president of the Second International Congress of Psychology in 1892 and presented there the recent findings of psychical research concerning crisis telepathy: the so-called Census of Hallucinations (Sidgwick *et al.*, 1894). It was not only William James but

[1] Compare, for example, the physiological theories of Hobson (1990) and Winson (2002) with the psychological theories of Van de Castle (1994) and Hartmann (1998). A good overview of the contrasting theories of hypnosis is to be found in Gauld (1992) and Kirsch and Lynn (1995).

[2] Although officially holding 'no corporate views', the SPR finally in 1990 issued their journal with a declaration saying 'for over a century the Society had published an impressive body of evidence for the existence of paranormal phenomena'.

many of the leading psychologists of the time such as McDougall, Freud, Coover and Boring, who had an interest in psychical research, yet, ultimately the two subjects went separate ways. The reasons for thus distancing psychology from psychical research and parapsychology probably reflects again, at least in part, the influence of political considerations: psychology could hardly be recognized as a normal science while it was tied to its occult double (Leahey, 1991). It was not until about a hundred years later,³ at the 27th International Congress of Psychology held in Stockholm in 2000, that the subject was again reinstated, this time in the form of a symposium organized by Robert Morris, Koestler Professor of Parapsychology at the University of Edinburgh (Morris, 2001). Looking at this history, the above-mentioned remit for this paper may suggest that with the emergence of 'Consciousness Studies', the earlier strategy of dissociating the new subject from parapsychology is in danger of repeating itself.

It might be thought that by now laboratory investigations of the paranormal would have revealed if there is a residuum of phenomena not explicable by occult beliefs, cognitive errors and magical tricks, and if so, would have forced psychology to deal with them. Yet stripped of its occult and magical trappings, there is still no clarity about what we are dealing with. Two of the classic works in this area from the twentieth century are: *Extrasensory Perception* by J.B. Rhine (1967/72) which summarizes the research of the Duke laboratory using the card guessing paradigm from 1930 to 1950, and its antithesis: *ESP: A Scientific Evaluation* by Mark Hansel. Hansel's volumes (1966/1989) are still quoted by many critics as having effectively demolished all the work of Rhine and his co-workers. Yet in order to do so, Hansel had to devise scenarios by which at least four of Rhine's star subjects could, all individually and on their own initiative, have cheated. For some scenarios to work the list was extended to at least some of his co-workers. To be fair, Rhine was always much aware of the need to take precautions against fraud so that formal testing required two experimenter checks along with duplicate recordings. Fraud has undoubtedly occurred in parapsychology on several occasions (Rhine, 1974a; 1974b), although for some readers it may seem a little fanciful to believe that so many cheats all congregated at one time at the Duke laboratory, while for others the possibility may still seem a plausible alternative to psi. Yet it should be said, despite the replication problems, Rhine's work did not entirely stand alone, and similar findings had been obtained by some researchers in the US (Mauskopf & McVaugh, 1980) and in England (see West, 1962). The unfortunate consequence of Hansel's critique was that some critics such as David Marks (2000) still eagerly accept Hansel's scenarios without further ado as probable enough to totally discount Rhine's results.

Had Rhine's results been readily repeatable, the critics would have been silenced long ago, but then it might also be said that had the phenomena been so easily demonstrable and separable from magic and mystique then they would

[3] Alan Gauld (1968, p. 147) notes that the Society for Psychical Research was involved in presentations at Paris 1889, London 1892, Munich, 1896, Paris, 1900. With the exception of a minor presentation by Sidney Alrutz, (the founder of Swedish psychology) at Geneva in 1909, the subject seems to have vanished from the agenda until the Robert Morris symposium at Stockholm, 2000.

hardly have needed to be caught in the laboratory in the first place. As for reasons for this replication difficulty, assuming the Duke results were not due to an epidemic of fraud, then perhaps we need look no further than the first two volumes of the *Journal of Parapsychology*. These contain several experimental reports directly concerned with how the ‘subject–experimenter relationship’ influenced results. Rhine would first argue for the importance of rapport between subject and experimenter. Later in life, after his own workers had difficulties in repeating the results, he would concede that the spectacular success during this period may have been due the unique enthusiasm and team spirit that was present in the early Duke laboratory.⁴ His co-worker Gaither Pratt described how in the initial testing period they discovered eight high-scoring subjects but two years later none were forthcoming. Pratt comments:

The sense of excitement and the adventure of scientific discovery were missing for the testers and the tested. To achieve the psychological equivalent of the earlier work, the participants should have been able to feel that they were helping to solve a real scientific problem, such as the occurrence of ESP had been in earlier Duke research. (Pratt, 1974, p. 153)

By the time Duke University laboratory (the predecessor of what has now become the Rhine Research Center) became independent, whatever magic was necessary for high-scoring subjects had almost disappeared.⁵

Was Rhine correct in maintaining it was all down to the charisma of the experimenter in motivating subjects or was this merely an after-the-fact rationalization? Besides the experimental work, which supports Rhine’s view (White, 1976a,b; 1977), history also speaks for it. The Duke laboratory was not, in fact, an entirely unique situation and whatever factors were responsible for its outstanding success, they were to reappear between 1962 and 1979 at the Maimonides Dream Research Laboratory in New York. Here again it is just possible that some difficult-to-define feature — at least in English — was at work. Indeed, the Scandinavian and German words *stämming* and *stimmung* may be more definitive ones since they refer to the atmosphere and feeling of a situation, and here the situation was such that a breakthrough in the current research paradigm appeared to be taking place. Naturally, the enthusiasm of the Ullman–Krippner–Honorton team, like that of Rhine and his co-workers before them, may have also been an important part of this successful atmosphere (Ullman *et al.*, 1974).⁶

[4] John Beloff (1993, p. 149) comments on this and it is interesting to compare Rhine (1948) and the Introduction to Rhine (1964).

[5] During the 1960s and 1970s some further three (Pavel Stepanek, Sean Harribance and Malcolm Bessent) were discovered by various laboratories and underwent extensive testing but unaccountably they were never used in any kind of demonstration in front of critics. My own experience of two apparently high-scoring subjects (Parker, 1974) unfortunately did not last long enough to achieve this goal but they did continue to score successfully for a short time in the presence of John Beloff and Brian Millar who had a psi-inhibitory reputation.

[6] The same could be said of the success of the remote viewing work carried out from 1972 to 1986 at Stanford Research Institute involving Russell Targ and Hal Puthoff and from late 1986 until 1995 at its successor, the Cognitive Sciences Laboratory, under the leadership of Edwin May (McMoneagle, 2000).

Of course, the critic might say new research paradigms merely introduce new sources of error, but the Maimonides studies have withstood the scrutiny of time, so much so that even Hansel, writing a later edition of his book (1989), appeared to have difficulties that may explain why he otherwise unaccountably persisted in claiming there were certain flaws in the procedure long after these have been shown to be non-existent (Child, 1985).

The Maimonides results are now usually simply ignored by the major critics such as Hyman (1996a; 1996b) and Marks (2000) or in the case of Alcock (1996), they are dismissed as non-replicable. Yet the 'failure to replicate' charge no longer seems to be well-founded. A recent review by Sherwood (2002) collected 21 reports of studies that could be seen as replication attempts of the Maimonides work concerning dream-ESP. Although as a whole the replication studies showed a lower effect size than the original work, there could be no doubt that the Maimonides findings were adequately replicated. There are naturally some weaknesses: Some studies included in the collection were not fully reported and most were conceptual replications with participants recalling their own dreams rather than, as in the original Maimonides studies, using the dream reports received through EEG-EOG awakenings. What is also again very obvious from Sherwood's analysis is that some experimenters were very successful, others not. All in all, the failure of the Maimonides work appears to be not so much a failure to replicate, but again a failure of further financing from mainstream science which would have been needed in order to make progress.

In the face of the pending closure of the Maimonides laboratory, Honorton began in the mid-1970s to work on a less expensive and less cumbersome technique than the EEG-EOG methodology. This became known as the *ganzfeld technique*, and the basic principle was much the same as with the dream telepathy studies: Spontaneous real-life experiences suggest that if telepathy occurs, it occurs most often when the 'receiver' is in an altered state (or at least in a relaxed state) and when there is an emotional crisis with the 'sender' (Stevenson, 1970). In the laboratory a homogeneous field of visual and auditory stimulation (the *ganzfeld*) induces a hypnagogic-like state in the receiver while the sender views an emotionally engaging film clip. Afterwards, the task is for the receiver or a judge to identify this film clip from decoy clips (usually three in number) that formed part of the set of four from which the clip was randomly chosen ($p = 0.25$).

The *ganzfeld* experiments began in 1964. They now number about a hundred studies, and have been conducted at some dozen different laboratories, so we should expect a clear answer to the question: Is the 'ganzfeld effect replicable? If we look superficially at all the experimental results, then the answer is an unequivocal, yes, but since not all the studies were done with the requisite controls against sources of error, inevitably the question becomes: Were all the successful studies merely replicating errors and were they open to fraud? It should be realised that very few studies were designed with the support of the resources needed to pay the double bill of being proof-orientated and replication-orientated. Nevertheless, in 1984 a constructive dialogue began between Charles

Honorton and the leading critic, Ray Hyman, resulting in a joint communiqué listing the more stringent criteria required for a replication to be declared as valid (Hyman & Honorton, 1986). It was important, for instance, to eliminate possibilities of multiple analyses, recording errors and, as far as possible, data manipulation. In what surely must be one of the most positive outcomes of any controversy in psychology, Honorton went back and redesigned the prototype manually operated ganzfeld, producing what is now known as the automated ganzfeld or *autoganzfeld* whereby these and other potential sources of error were eliminated. He and his colleagues then went on to carry out ten autoganzfeld studies with eight separate experimenters. These, the so-called PRL studies, took place at the Princeton Psychophysical Research Laboratory, and produced a 32 per cent hit rate where chance would be 25 per cent ($z = 2.89$, $p = .004$, two-tailed, with a small-medium effect size: $\delta = 0.59$) (Bem & Honorton, 1994).

It might be thought that the issue was now settled. However, the report by Daryl Bem and Charles Honorton detailing these findings, published in *Psychological Bulletin* shortly after Honorton's death, did little to attract funding or any interest from mainstream psychologists (Bem, quoted in Parker, 1994). Worse, the next 30 studies selected for a further meta-analysis by Julie Milton and Richard Wiseman not only failed to replicate the PRL effect by being overall non-significant, but produced a non-significant near-zero effect size (Milton & Wiseman, 1999). It may sound contrived but it is conceivable that many experimenters were by then tiring of straight replications so that by deviating too much from the original recipe, they failed to produce the requisite effect since there was a demonstrable lack of homogeneity both in the designs and in the results. Moreover, the overall statistical significance of the meta-analysis did return with the next ten replication studies, although the effect was still much smaller than with the original PRL studies. The homogeneity argument does, in fact, have strong empirical support in the form of an elegant study reported by Daryl Bem, John Palmer and Richard Broughton (Bem *et al.*, 2001). The authors recruited senior students to carry out blind ratings of the degree to which the post-PRL studies deviated from the standard ganzfeld procedure, and found a clear relationship between standardness and statistical significance, with the more standard studies showing a joint effect size compatible with the earlier studies.

The Return to the Status Quo

In 1995, the Clinton government commissioned Jessica Utts and Ray Hyman to assess the application value of parapsychological research findings. They concentrated on the research design known as *remote viewing* (localization and description of remote targets via ESP) and on research with the ganzfeld technique.

Both assessors agreed that there existed an *anomaly* in the sense that there was no apparent explanation for the results. Utts argued that psi ability had been established while Hyman insisted that all that could be concluded was that an anomaly existed in the form of an inexplicable finding, thereby avoiding admitting, along with the anomaly, the accompanying heavy baggage from psychical

research. They differed in that Utts was of the opinion that more replications were meaningless while Hyman thought, given the ambiguity, they were still appropriate. Despite their differences both Hyman and Utts agreed on what else was needed. Utts (1996) wrote:

Resources should be directed to the pertinent questions about how this ability works. I am confident that the questions are no more elusive than any other questions in science dealing with small to medium sized effects, and that if appropriate resources are targeted to appropriate questions, we can have answers within the next decade.

Hyman (1996) wrote:

Despite better controls and careful use of statistical inference, the investigators seem to be getting significant results that do not appear to derive from the more obvious flaws of previous research. I have argued that this does not justify concluding that anomalous cognition has been demonstrated. However, it does suggest that it might be worthwhile to allocate some resources toward seeing whether these findings can be independently replicated. If so, then it will be time to reassess if it is worth pursuing the task of determining if these effects do indeed reflect the operation of anomalous cognition. This latter quest will involve finding lawful relationships between attributes of this hypothesized phenomenon and different independent variables.

In one respect Hyman may have been insufficiently critical. Some of the earlier pre-PRL work was carried out by Carl Sargent, a Cambridge University researcher whose results were considered suspect by a critic, Susan Blackmore. The debate was intensive and became a major reason for Blackmore's subsequent disillusionment with the field (Blackmore, 1987; Parker and Wiklund, 1987; Sargent, 1987). The issue was never fully resolved and both individuals have now left academic psychology for more lucrative work. Although these particular questionable results now make little difference to the results of the current meta-analysis, if we wish to reduce the possibility of the data manipulation and procedural errors then a separate analysis should be made of the high-quality autoganzfeld studies.⁷ Unfortunately, the development of the *autoganzfeld* put the methodology back in the expensive and cumbersome class, so, following Princeton, only three laboratories in the world (the Rhine Research Center, and parapsychology laboratories at the Universities of Edinburgh and Amsterdam) have used it. Nevertheless, based on the list provided by Bem *et al.* (2001), it can be seen that there were eight such 'well-conducted' replication studies (scoring 7.0 on a seven-point scale of standardness) involving four teams which produced a Stouffers z of 3.06, $p = 2.2 \times 10^{-3}$ (two-tailed). What is again obvious from looking at the z values for the 'well-conducted' studies replications is that there are clear differences between research teams and experimenters, and much of the success is still dependent on particular experimenters.

[7] I am aware that by this I am excluding some of my own successful ganzfeld work (Parker, 2000). Although we took reasonable precautions and the results were not dependent on any one experimenter, from the critic's point of view they do not have all the precautions included in the autoganzfeld.

Given this success, albeit a very limited one — but perhaps also an accredited one — and given the limited resources available, it would seem that Hyman's (1996a) demand for yet more replications has in principle been met during the five years following his report and that it is now time to look for lawful relationships. This is a conclusion even supported by the critics Milton and Wiseman (2001) in their latest comment about the outcome of the psi-ganzfeld meta-analyses in the *Psychological Bulletin* debate — a debate which began in 1994 and sporadically continues. In the same issue of that journal, Lance Storm and Suitbert Ertel (2001) maintained that the psi-ganzfeld database, seen as a whole, is now both consistent and convincing as regards to a replicable effect. In replying to this claim, Milton and Wiseman naturally questioned the quality of many of the studies but they also constructively suggested that the way forward was now to strive towards the Beloffian goal of being able to demonstrate psi where and when required. Claiming that nothing was actually known about the variables determining the occurrence of psi, Milton and Wiseman then suggested the next step 'would be to assess critically the experimental evidence for the effects of many variables that have been suggested as affecting outcomes in ganzfeld studies' and given this, a demonstrable 'replicable effect would be possible.' (Milton and Wiseman, 2001, p. 437)

Hyman (1996b) would say not even this would be enough:

Even if . . . we were to find we could reproduce the findings under specified conditions, this would still be a far cry from concluding that psychic functioning has been demonstrated. You need a positive theory to guide you as to what needs to be controlled, and what can be ignored.

Of course, some readers might think that Hyman was now moving the goal posts, but for the moment what counts most is that there should be some forward movement of the play. This has not been the case. Despite Utts's strongly worded recommendation for university-based funding and Hyman's support in principle for theory-driven research, there has been no tangible response to this in the form of research grants or government-funded projects (Utts, 2002). Indeed, Hyman's remarks may instead have supplied a needed catch-22 to bring the debate back to status quo: the phenomena must first be given a theoretical explanation before research into their nature can be funded. A good illustration of this was found recently in a Swedish Science Research Council's assessment of a project that sought to look for some of Milton and Wiseman's 'lawful relationships' using psi in the ganzfeld. Although the application was approved, the project was given a low priority rating because information was first required about 'the mechanisms, which are the basis for the phenomena,' and about how 'that information is transferred between sender and receiver.' (SRC, 2001)

At the stage when the amount of work done in parapsychology has been calculated to be equivalent to no more than two months work in psychology (Schouten, 1998) the prior demands for precise and established theories are not only unrealistic but amount to little more than a catch-22. If efforts comparable to those in parapsychology had been deployed to areas of arguably comparable

complexity, such as hypnosis or dream research, then at best we would have expected little more than we have here today: a *prima facie* case for the phenomenon's existence and the development of appropriate research methodologies to study it.

One of Hyman's main arguments for conceding the presence of an anomaly concerns an argument put forward by David Marks for a 'ten-year rule' of delay from the time from when strong psi findings are presented to when normal explanations for it are finally thought of. This division into parapsychologist and critic is an unfortunate one because it places the critic forever in the role of a hacker who is given ten years to find a way of breaking into the computer security system. Even so, it is now twenty-five years since psi-ganzfeld research began and there exist other, older successful experimental designs such as Maimonides dream work (Ullman *et al.*, 1974) and hypnosis (Honorton & Krippner, 1969) which have stood an even longer test of time. Marks' argument fails then by virtue of its own criteria.

In taking a purist view by claiming all that has been demonstrated is an anomaly in the ganzfeld setup, it should be pointed out that critics such as Hyman are also ignoring that while much of the ganzfeld work has been process orientated, there are *proof-orientated psi experiments* (see, for example, Schmidt *et al.*, 1986) as well as the *conceptual replications* carried on psi in relation to other altered states (see Alvarado, 1998; Parker, 1976). The ganzfeld work has, moreover, an *ecological validity* in being an experimental analogue of themes re-occurring in the historic case collections of psychical research (Gauld, 1968; Stevenson, 1970; Roy, 1996). Case collections originally gave impetus to the ganzfeld and Maimonides designs by indicating that psi is most frequently associated with dream states rather than waking states, and ESP in the dream state has more information content (L. Rhine, 1962).

In down-playing the value of the well-documented case collections, leading critics such as Marks, Hyman and Alcock are naturally influenced by their backgrounds as social and cognitive psychologists. Certainly, many sceptics writing in the 1700s and 1800s knew about human proneness to self-delusions and imagination but today we have a greater precision in describing the sources of error through such terms as selective memory, contextual cues, fantasy proneness and subjective validation. James Alcock (2002) succinctly expresses this view as: 'If telepathy did not exist the human mind would need to invent it'. The problem, of course, with this statement is that some philosophers would say the same thing about the external world. Worse, there is a risk that cognitive psychologists, like psychoanalysts before them, have already begun to misuse terms like cognitive defences and subjective validation as (to borrow from what William James had to say about the Unconscious) 'the sovereign means for explaining away anything' that contradicts cognitive theory.⁸

In any case, although case collections are important, the primary thrust of parapsychology today is in laboratory experiments which produce quantitative evidence. Ironically, at least one critic seems to agree that the efforts to produce the above pure quantitative evidence may have contributed towards this distrust

of what the findings actually mean. James Alcock (1996) lists eight reasons for being sceptical about ESP. Several of these actually concern the replication and methodological shortcoming discussed above, and a further two reasons concern the lack of theoretical understanding and progress, which will be discussed later. A final reason for being sceptical that Alcock gives relates to Hyman's refusal to concede more than a mere anomaly, in that: 'the best modern evidential claims are hidden from easy view in the world of statistical analysis' (Alcock, 1996, p. 249). Indeed, if ganzfeld psi is more than an anomaly and is indeed evidence for some form of little-understood communication, then obviously it should have information content. This consideration formed part of the impetus to develop a new form of ganzfeld experimentation which we have installed at the University of Gothenburg in Sweden and lately at the Institute for Frontier Areas of Psychology at Freiburg, Germany.

The Content of an Anomaly: Recent Developments with the Ganzfeld Technique

In designing experiments, the above review suggests it is not enough to be solely technically orientated and that experimenter characteristics as well as participant factors are important. To see what factors must be included in psi experiments, let us use the example of the ganzfeld. Like Rhine before him, Honorton attributed his success with the ganzfeld to the importance of factors in the experimenter such as warmth and empathy towards the participants and the ability to motivate others (Terry & Honorton, 1976). Some of these statements might be regarded as after-the-fact rationalizations and indeed some critics (Milton & Wiseman, 2001) have asserted that nothing is known *empirically* about what makes a successful psi experimenter and what the psi-conducive factors are in experimentation. This would imply that 50 years later we must begin again where Rhine left off. Fortunately, several reviews of the empirically based research literature give strong support to the hands-on-based impressions of Rhine and Honorton (Kennedy & Taddonio, 1976; White, 1976a; 1976b; 1977). The assertion becomes more puzzling when it is well known that Wiseman has himself contributed to this literature (Wiseman & Schlitz, 1999), his own findings actually supporting Rhine's observations.

What appears to be known is that in some cases it seems that as well as a psi-conducive effect, a psi-inhibitory effect could also occur in which participants could consistently score significantly below chance — so-called

[8] The area of spontaneous cases is a relatively neglected by parapsychologists and more or less totally ignored by psychologists. Moreover, few of the traditional explanations from cognitive psychology would seem to apply to the better cases (Parker, 2002). For instance, Gauld and Cornell (1979, pp. 254–6) report evidence suggesting that witnesses over a long time period actually tone down rather than exaggerate their accounts. Neither are all mediumistic successes only so-called Barnum effects — general statements that that receive wide endorsements. Playfair and Keen (2003) report a contemporary mediumistic case which gave such specific information in terms of correct names and other specific details that the police reluctantly took her testimony seriously. Her identification of one the suspects was later confirmed by DNA testing.

psi-missing. This bi-directionality of psi effect is reminiscent of an effect found many years ago in psychotherapy — that the wrong therapeutic conditions could make patients significantly worse than a non-treatment condition (Truax & Carkhuff, 1967). Another apparently important factor finding concerns the *sheep-goat effect*, referring to the participants' degree of personal belief in psi and own experiences of the phenomena and how this has on many occasions been used as a predictor of scoring (Lawrence, 1993; Goulding & Parker, 2001).

The problem with such findings is that while they may appear to be at best necessary conditions, they never are sufficient conditions for psi to occur and, worse, they can be misused as an escape clause for explaining why experiments fail. Accordingly, in designing a program of research with the ganzfeld at Gothenburg, we intentionally selected individuals who scored high on the *sheep-goat scale* (of paranormal belief and experiences) with the expectation that those with more of their own psi-like experiences will score better, and because of the above considerations we always predict that experimenter effects will arise. Both these sets of predictions have found support in our analyses (Parker, 2000).

In 1995 our own attempt at replicating the PRL work was for financial reasons limited to using a manually operated video system of showing target film clips. Following the termination of the ganzfeld session, the participant would compare the mentation report with each of the target film clips previously recorded on a composite tape, and then make a direct forced choice as to which of the four clips was the target film clip. In the course of this work, by simply recording the mentation report along with the film track, we began to observe that some of the mentation reports of the imagery during ganzfeld appeared to be describing *in real time* whole sequences of the changing content of film clips. The critic in us would say this is merely our subjective validation in seeing what we wanted to see. Yet some descriptions were very accurate and the real time aspect seemed even harder to dismiss.

In order to document this, at the suggestion of a colleague, Dr Jan Dalkvist at the Psychology Department in Stockholm, a second video player was added which would copy the film clip as it was viewed and at the same time receive a sound recording from the ongoing mentation report of the participant in the ganzfeld state. The resulting audio-visual recording would then give us real-time recording of the film clip and mentation report and could be evaluated for real-time correspondences. Since the results of this set of five experiments were statistically highly significant ($z = 3.02$, $p = .0025$, two-tailed, $ES = .25$), it seemed legitimate to look at this content at least from the point of view of generating hypotheses about the lawful relationships sought after by critics and parapsychologists.⁹ About one in five or six sessions would produce a mentation report which seemed to be describing the film clip in real time. We reasoned that

[9] To avoid a file drawer problem it should be mentioned that one further uncompleted study involved inviting back ten previously successful participants. Although immediate re-testing was very successful in our third ganzfeld study (Parker, 2000), this time only five hits in 29 trials were obtained but it should be said two years had now passed and many relationships had altered.

rather than studying an anomaly, this would provide an ideal opportunity for studying high-quality psi when it was actually happening. What speaks against simple subjective validation are the cases where not only unequivocal details are given but the mentation appears to follow the changing and unpredictable scenes in the film clip. For instance, in the session illustrated below, the receiver in ganzfeld gave a whole sequence of responses which not only correctly described a woman first in a marsh area and a forest with enormous trees but went on to give other responses that seemed to be misperceptions of the film content: a snowy effect (snow plough), the coloured wigs (coloured balls) her women assailants had on, and the boomerang-shaped stick she used to defend herself. All of these responses were in real time and culminated in the scene shown in Figure 1 when she said 'someone falls hitting their face on stony ground'. It should be added that the film has only music as a sound background, had not been used before and the participant had no prior access to it. Obviously, such illustrations are not meant as proof of psi but they might suggest there is more content to the anomaly than statistics.

It was these kinds of results that gave impetus to a co-worker, Dr Joakim Westerlund, and myself to develop what is now known as the digital ganzfeld. The digital ganzfeld has major improvements over the earlier autoganzfeld, in that it not only enables the real time recording of the mentation report with the film clip to be made, but the recording of the mentation report can also be superimposed onto the decoy clips as well as onto the real target film (see Figure 1). This provides the opportunity of using the real time matches as an important aid



Figure 1

Video real time recording: The participant described accurately in real time with the film a whole sequences of events including: 'someone falls and hits their face on stony ground' at the exact moment when this took place.

in determining which is the target film. From the point of view of facilitating replications and involving mainstream psychologists in ganzfeld research, the procedure has something further to offer: The digital ganzfeld is a portable system which, when placed on a laptop computer, can be used anywhere in the world.¹⁰

A legitimate use of such qualitative material is to generate hypotheses about how psi enters consciousness. Initially we collected about 20 examples of what seemed to be high-quality, real-time psi but what was more interesting, from the point of view of hypotheses, were the near misses that interphased with sequences of accurate descriptions. Very often these seemed to be evidence of *top-down processes* in which the participant was struggling to interpret vague information that

he had to work on and thus the process could be likened to normal sensory input during non-optimal conditions. It appeared to be the case that the theme would be correct but interpretative mistakes would then occur such as when the film clip suddenly focused on an imp-like creature hidden amongst seaweed-covered rocks and the participant responded with 'ivy-covered rocks with

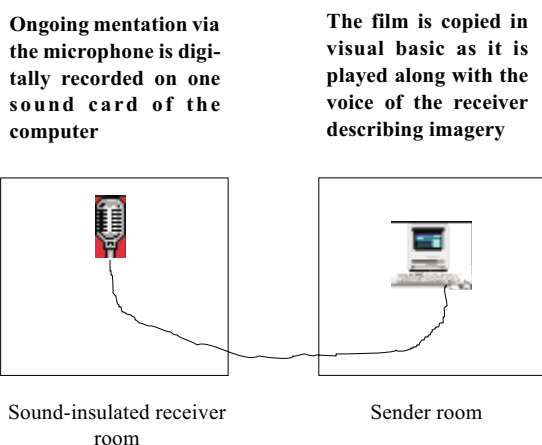


Figure 2. The Digital Ganzfeld setup.

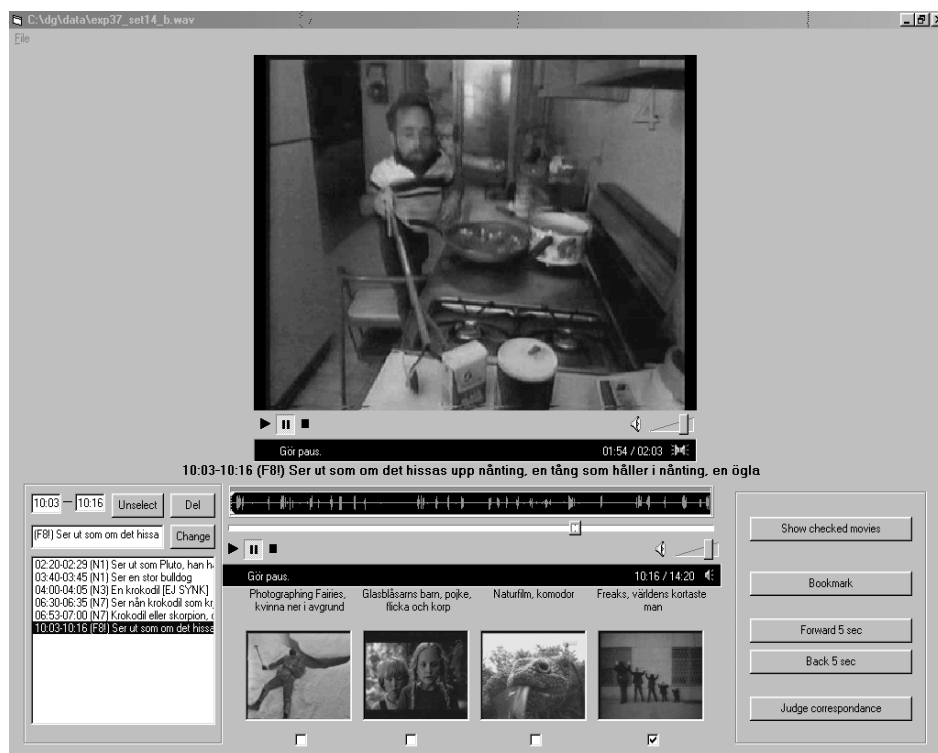
something in it' or such as when jumping white lemurs were seen as 'jumping white lambs' (Parker *et al.*, 2000).

One of the difficulties in evaluating this kind of qualitative material is of course its selective nature and clearly we cannot go much further until some means of quantifying the material is developed. A major obstacle in reaching that goal is that of dependency: If participants guessed correctly, for instance, that the clip depicts a forest scene then some other features such as birds and paths, and for some people even threatening scenes, like the one occurring in Figure 1, might be expected to follow. Of course there are problems with using this kind of argument to readily dismiss the above findings. Although such correspondences are conceivable, as was mentioned earlier, it would seem unlikely that such correspondences would be constantly in real time, and the most impressive examples concern the correspondences between ganzfeld mentation reports and the sudden and unexpected events in the film clips. The argument can, of course, also backfire in that if the initial guess happens to be wrong, it can lead to a string of wrong associative responses. Ironically, it is the complexity of this dependency

[10] However, when set up on a stationary computer system in order to prevent any leakage, we use the extra precaution of separate sound cards for the recording of the mentation report and the film sound.

aspect that seems to have escaped the want-to-be critics. David Marks (2000) is the avowed spokesman for the concept of *subjective validation* in explaining away such coincidental ‘hits’ both in experimental work and real life. Clearly there is a need to support this with figures and Marks calculates that 4,950 pairs of events can be cross-matched for a given person in a given day. To arrive at this figure, he makes the gratuitous assumption that there are a hundred independent events experienced by a person in a given day, but like the content of films, surely few people hopefully have such entirely random lives.

A simple and better solution to the above problem, proposed by another of my colleagues, Dr Joakim Westerlund, is for a judge to first identify what *appear* to be sequences of real-time matches by the usual procedure of blind matching the mentation report against both targets and decoys. Further selections are then made amongst the apparent real-time matches: the matches have to be limited to a single phrase with a maximum of one best match per film clip. All such best matches in a given experimental series then form the collection which is used for further



↑
List of bookmarks, each referenced to selected sequences in the film-clips of what might be real time psi.

↖ ↑ ↗
Film-clips can be activated here and compared one or two clips at a time with the content of the mentation report.

Figure 3
Real time digital recording:
Capturing psi (or is it capturing subjective evaluation?) when it happens.

quantitative study. If the matches are due to psi, rather than subjective validation, then we would expect the collection to be formed mostly from the target clips rather than from the decoys. This solution, although ingenious, has its limitations since by reducing the numerous striking correspondences found in good-quality hits to one singular phrase, a crude measure is produced involving a major loss of information, and even within single phrases some dependency can remain. The method is nevertheless a start and can be somewhat improved when objectively defined criteria are set up for rating the quality of real-time correspondences. The process of identifying the representative units for good hits is actually programmed into digital ganzfeld by using a bookmarking system: The apparent real-time correspondences are listed so that when a particular item on the list is activated, the sound from the mentation record is recalled along with the corresponding part of the film clip. An example is found in Figure 3, where the participant says: 'looks like something is being lifted, a tong which holds something'. The statement is synchronous with the five-second sequence in the film-clip when a handicapped man suddenly appears, lifting an object with the help of a type of tong.

To date, because of shortage of funding, only two main experiments have been run using this technique. An additional feature for these experiments was the use of two film clips, each lasting nearly 15 minutes per session; a modification which radically improves the statistical power of the procedure. In the first study (Goulding *et al.*, 2003), 64 such double sessions were run, giving 128 trials (each having an expected 0.25 p value). Time restrictions meant that participant judging (the usual procedure with ganzfeld which we used throughout our earlier studies) could only be carried out on half the sessions. The more systematic and time-consuming judging of the whole mentation report against each of the four (target and decoy) film clips had to be carried out by an external judge. One major purpose of the experimental design was to determine if this form of external judging with its more systematic use of bookmarks would be superior to participant judging.

Another of the main aims of the study was for other experimenters to independently replicate the earlier highly significant findings which my co-workers and I had previously obtained (Parker *et al.*, 1997; Parker *et al.*, 1998) using the same type of participant population. However, this time, the same method of participant judging gave scores *significantly below* chance expectation — a so-called psi-missing effect — with the direct hit rate lying at 14 per cent, (MCE 25 per cent), $z = 2.02$, $p = .05$ (two-tailed). External judging, which was the basis for the hypothesis of the experimenter, Anneli Goulding, was on the other hand close to chance with 23 per cent hits ($z = .041$). In accordance with the above method of quantifying the qualitatively good hits, a collection of 18 apparent real-time correspondences was formed. These were identified to occur more often with the target film than controls, although not significantly so: 33 per cent compared to the 25 per cent chance expectancy. When friends were in the role of being the sender, a *post-hoc* test of the judge's rankings gave a 45.5 per cent hit rate (chi-square = 7.2, $df = 3$, $p = .066$, two tailed) with the target ratings then showing a significant effect ($F = 4.54$, $p = .005$).

Clearly these type of results — and they are unfortunately all too common in parapsychology — can either be ignored, or dismissed as a peculiar statistical artefact, or we can try to make sense of them. If the psi-missing findings are a genuine effect, then it is conceivable that the systematic nature of the external judging did actually succeed in moving the rankings and ratings in a positive direction but not beyond the chance level. By using only the judge's blind identification of the best bookmarks, it might be that we had raised the receiver's original hit rate from 14 per cent to 33 per cent. Currently, we are assessing this qualitative material in order to determine whether the hits do in fact differ from misses in a quantifiable way.

In this particular case it is not difficult to think of possible causes of the apparent psi missing, such as the fact that the frustrating delays that occurred due to programming problems meant that experiments were run on a stressful, very tight time schedule.

The second study (Wright, 2003), which has just been completed at the time of writing, used only receiver judgments and produced a scoring rate more than twice as high compared to the above. The results were then in line with what has previously been claimed for the Ganzfeld: a 32–33 per cent direct hit rate and a small to medium effect size (Cohen's $r = 0.17$, $z = 1.48$, $p = .09$). Of particular relevance was the finding that one session per day trials gave a 45 per cent hit rate while those with two sessions per day gave a mere 19 per cent hit rate ($\chi^2 = 5.4$, $p = .02$) thereby confirming a previous finding of Sandow (1979); findings that are consistent with the hypothesis that stress and tight schedules negatively effect psi performance.

These findings unfortunately also illustrate what appears to be an intractable problem in parapsychology. Until we can predict such outcomes *ahead of time*, the establishment of lawful relationships still evades us. What is very clear from the above review and from other more extensive reviews (e.g., White, 1976b) is that experimenter effects and psi-conduciveness are every bit as integral a part of the phenomena being studied as, say, placebo effects are in psychological treatment. But there is some evidence that they are even more so.

Returning the Magic to Psi?

In one respect it seems clear that Rhine underestimated the difficulties that lay ahead. For whatever reason the phenomena appear to have an *elusiveness* as a defining characteristic that makes them intrinsically difficult to capture in the laboratory in a stable, predictable and controllable fashion (Kennedy, 2001). There are many findings like the above where an expected significantly positive effect in an attempted replication suddenly reverses and becomes a mirror image of what is expected (see Parker, 1978 for a review).

That this elusiveness sounds almost magical may not be entirely facetious. It is just possible that the association of psi with magical practices and mediumship is not a sign of its being entirely illusory but that such ambiguous circumstances may actually be necessary psi-conducive conditions. The late Kenneth Batchelor, as a chief consultant clinical psychologist with the National Health Service in England,

had some credibility to risk when he claimed to have conducted many successful experiments in large-scale psychokinesis. Batcheldor would first produce some of the phenomena artefactually but then discovered that genuine phenomena would actually also spontaneously seem to occur. Success was attributed to the ambiguity around who was responsible for the phenomena which allowed the release of a creative psi ability in the participants (Batcheldor, 1994).

While this may seem a preposterous let-out, interesting in this context is endorsement of genuine psi by professionally experienced magicians, which is reported to be between 72 and 87 per cent (Truzzi, 1997; Hansen, 2001). This is a remarkable contrast to that of psychologists which, at an estimated, 34 per cent, must be a record low amongst scientists (Wagner & Monnet, 1979 quoted in Bem & Honorton, 1994). Although magicians can claim to be true experts in applied cognitive psychology, obviously even they can deceive themselves and be deceived by other magicians. However, given this expertise, such findings clearly warrant further thought since they may reveal something of central importance about the conditions conducive to psi. Might it just be that in pursuit of making psi clinically clean by bringing it to the laboratory, we are removing some of the essence of the phenomenon in terms of what makes it readily happen? Recently, Matthew Smith and Michael Gordon investigated the psychology of the 50 named 'psi-conducive and psi-inhibitory experimenters' and found by multiple regression of self-report questionnaires that 'higher psi-conduciveness scores were associated with belief in one's own PK [psychokinetic] ability and belief that it is possible to demonstrate ESP in an experimental study' (Smith & Gordon, 2001).¹¹

The mention of such alleged phenomena as mind-matter interactions known as PK or psychokinesis may strain the reader's sense of what is credible. Nevertheless, the Rhine laboratory had already in the 1940s begun testing the claims of gamblers to be able to influence throws of the die and this became extended from the 1970s to influencing the output of random-number generators. The database from all these experiments is now, by parapsychological standards, so massive (597 studies) that meta-analysis produces enormous odds against chance whereas the control studies (235 studies) do as we might hope, confirm chance expectancy (Nelson & Radin, 1989). This sounds impressive, at least until one realises that the apparent PK effect is only about one per cent above the 50 per cent chance expectancy and might be taken as some unknown anomaly or artefact, were it not again for the occasional well-documented cases of large-scale spontaneous phenomena. Probably the most famous of these is the Rosenheim case, investigated by Hans Bender of Freiburg University, and concerned the numerous electrical disturbances and movements of objects that appeared to centre around a young woman. Such explanations as fraud seem rather facile when confronting the full force of German meticulousness and efficiency thrown into the case, a force which mustered police from the Criminal Investigation Department, staff and personnel from the Municipal Fire Service and Electricity

[11] Some empirical support is found in a review by Brian Millar (1979) who concluded that considering psi ability is rare, psi-conducive experimenters were themselves to be found over-represented as psi-conducive subjects!

Department as well as physicists from the Max Planck Institute. All of those involved in it failed to reveal anything suspicious or any normal explanation. In addition, there were about 40 first-hand witnesses to the events and there appear to have been brief film documentations of some of them (Bender, 1974). If this was fraud, it would have to be one of many such inscrutable cases. Even Charles Honorton reported and filmed with the help of an amateur magician some apparently genuine PK effects by one of his former assistants, Felicia Parise, which resulted in the observable movement of objects (Honorton, 1993b).

Accepting PK would, of course, render the world a much more plastic place but it would also explain some of the inherent difficulties in producing phenomena to order, since not only do the expectancies of the experimenter but also his own psychic abilities become an important part of the experiment. J.B. Rhine to my knowledge rarely if ever commented publicly on the psi ability of the experimenter, but in 1975 in a private letter to me, he replied to my questioning by expressing the following viewpoint:

I do not think the experimenter's own psi ability has shown up to be as an essential factor, but I still think we must assume that every experimenter has the ability and only needs to learn how he himself can liberate and register it. However, the very conditions that help the experimenter to liberate this might be the kind that would help him to induce his subjects to perform successfully (Rhine, September 5, 1975).

So perhaps Rhine, in his later years, did realise something of the complexity of the area that he had entered. Yet, this concept of psi as pervasive and elusive phenomena coupled to experimenter effects might suggest that psi, if it exists, is an intractable problem lying completely outside of science. This would be wrong. The experimental work and spontaneous case studies reviewed suggest that ESP-derived, definitive units of information are, as they enter consciousness, fully amenable to study, and the large-scale PK effects, if they are genuine, are quite tangible and offer an ideal opportunity to study mind-matter interaction. What we should not do is to be deterred if we still do not have the know-how to make psi happen at will. We should also not forget that at least some of the apparent elusiveness might simply be due to our ingenuity in always being able to find alternative explanations for psi, and thereby futilely pursuing the final crucial 'proof'. Of course, the extant evidence could still be an illusion, but this would seem to be an ambiguity, albeit remote, that we may have to live with in order to determine if progress can be made.

Perhaps if we were to do like Batchelder and give our participants a mixture of correct and false-positive feedback, thereby creating an atmosphere where a 'miracle' is expected, effect sizes might soar. The very least we can learn from this work is the importance of being more adventurous in our experimental designs. Experimenters with charisma and charm rather than psychology degrees could be employed to run testing sessions whereas those with doctoral degrees retain responsibility for seeing that the formal controls inbuilt in the procedure are followed and ensure that the ethical standards for interactions are met.

Conclusion

Many critics and parapsychologists would agree that there is an anomaly to be investigated and hopefully at least some of these critics, after reading the above review, might even agree that the anomaly appears to have both information content and dynamic influence. Indeed, the divisions between critics and parapsychologists is a false one, nurtured in the past by dogmatic and dismissive statements rather than facts. However, if the balance or consensus that has been reached is to become a platform for progress rather than for maintaining the status quo, I would argue that there needs to be some realisation that we have come as far as we can in asking the question 'Does psi exist?' and need to ask new ones. The question that would appear to be most appropriate to critic and parapsychologists alike becomes: Assuming ESP ability exists, can we make its occurrence more likely and thereby learn something about its nature?

Given the complexity of the phenomena, two further prerequisites for progress would be the development of new designs that capitalize on psi's apparent elusiveness and the deployment of appropriate resources needed for such research programmes.

Regrettably, there are many reasons for supposing this scenario will not happen, not the least the unlikely alliance between science and religion in seeing the claims of the occult as at worst a mutual enemy, and at best an unwelcome guest. It might easily be thought that religion would have much to gain from research in this area — and indeed one leading critic (Alcock, 1987) depicted parapsychology as 'the search for the soul'.¹² Paradoxically, many of the claims of the paranormal are still seen as either a devilish challenge to Christianity or as an affront to Humanism. Some scientists may even have a hidden agenda in defending one or the other or both of these faiths, resulting in the absurd situation where some critics, as Honorton (1993a) aptly puts it, are more inclined to believe in angels than in psi.

There may be other, more subtle reasons for the failure to resolve the challenge presented by parapsychology. Ray Hyman (1996), for instance, partly justifies his scepticism on the basis of a rewrite of contemporary physics being required in order to accommodate the phenomena (although I fail to see why advances in one area of science have to be constantly held congruous with knowledge in other areas). Ironically, in terms of rewriting, the opposite may be true. Given that parapsychology has arguably survived the most intensive and prolonged scrutiny that any area of psychology has been subjected to, then such a declaration should imply that human self deception is so devious and pervasive that much of psychology, and perhaps even parts of natural science, should by rights come into question. Rupert Sheldrake (1998) surveyed papers published in leading science journals for their attention to experimenter effects and blind assessments of data. The highest proportion was by far to be found in parapsychology (85 per cent) followed by medical sciences (six per cent) and psychology (five per cent). Many

[12] One leading parapsychologist, Charles Tart (1987) in the peer debate with James Alcock (1987) found nothing at all wrong with this arguing that many major enterprises in science had begun with a search for something.

findings considered established in psychology, such as the classical Asch experiments influence of group pressure on conformity judgments, suffer from replication problems (reviewed in Alcock *et al.*, 1998). It would therefore seem inescapable that if the same criteria used for rejecting parapsychology are applied to psychology, then many, if not most, areas would fall. It would be strange then if there were not strong forces that wish to maintain the status quo and let the issue remain, as it is, unresolved.

The alternative scenario, that genuine phenomena exist, may be seen as equally unwelcome since it may open the door to psychical research with its whole troop of unwelcome phenomena from mediumship to apparitional experiences — or worse, New Age — although the paradox is that experimental parapsychology provides the means of dealing with the unbidden guests by explaining away most of them as forms of psi from the living. This scenario is obviously not without its risks but has some potential benefits as regards the status of psychology amongst the sciences: Many authorities (e.g., Tart, 1997) agree this would bring about a re-evaluation of the function of consciousness in nature and provide new perspectives on the mind–body relationship.

Clearly, the possibility of developing theories which will resolve the issue *does* lie within the grasp of our generation. As shown earlier the advance of technology enables us now to study what at least appears to be high-quality psi. Considering the complexity of factors involved, further experimentation with the backing of mainstream psychology might or might not make the kind of rapid progress that Jessica Utts (as quoted earlier) foresees but there is no doubt it would resolve, once and for all, the basic issue of what we are dealing with. Of course, the apparent elusiveness and experimenter-psi effects make the phenomena unique in science, but to argue that this is beyond science is to repeat what was once said about psychology little over a century ago. It probably only means we have to be more sophisticated and ingenious.

Alan Gauld concludes his scholarly review of the history of hypnosis with poetically eloquent advice that might apply equally well to parapsychology:

The mysterious domain [of hypnosis] emerges, then, as a kind of fairy palace, less than real, but more than illusion. It has, one may say, sufficient substance in its foundations to have deceived mortals rather well. Especially has it ensnared savants of past generations, who in turn have misled the media and the public at large. But from our vantage point of the end of the twentieth century we can begin to see that there is no one path by which it may be reached, no one material of which it is built, no one hidden chamber containing all its secrets, no one key that will open all its doors, and no simple formula by which it may be dispelled. Those who set out to investigate it should be beware of the bafflements to come. (Gauld, 1992 p. 624)

The reassuring aspect is that with sufficient persistence and backing, progress does now seem to have been reached with hypnosis (Kirsch & Lynn, 1995; Oakley, 2002). So if we are serious about wanting to understand psi, the above-reviewed findings may give us reason to believe that by liberating ourselves from simplistic questions, and applying efforts and a technology appropriate to the complexity of the task, we can at last come further than our immediate

predecessors, or even our ancestors, at understanding the nature of these phenomena. High-scoring subjects and successful experimenters are to be found and a technology is available.

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